

PHILOSOPHICAL  
TRANSACTIONS.

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I. *A Second Paper on Hygrometry.*

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IN a Paper which I had the honour to present to the Royal Society in the year 1773, I sketched the following propositions, as fundamental for the construction of an *hygrometer*. 1<sup>st</sup>, That *fire*, considered as the cause of *heat*, was the only agent by which *absolute dryness* could be *immediately* produced (§ 5.). 2<sup>d</sup>, That *water*, in its liquid state, was the only sure *immediate* means of producing *extreme moisture* in hygroscopic bodies (§ 8. and *seq.*). 3<sup>d</sup>, That there was no reason, *à priori*, to expect, from any hygroscopic substance, that the measurable

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effects produced in it by *moisture* were proportional to the intensities of that cause; and, consequently, that a true *hygrometrical* scale was to be a particular object of inquiry (§ 2.). 4th, lastly, That perhaps the comparative changes, of the *dimensions* of a substance, and of the *weight* of the same or other substance, by the same variations of *moisture*, might lead to some discovery in that respect (§ 72.). The same propositions will be the subject of this Paper.

### *Of absolute dryness.*

1. An *hygroscopic* body, which is not brought into contact with any other body *drier* than itself, cannot lose any part of its *moisture* but by *evaporation*; and if this is intirely produced by *fire*, there may be such a degree of *heat* as will cause the total *evaporation* of that *moisture*. This is the principle on which the above first proposition was founded; but at the same time I mentioned, that I had not brought it into practice, because of the impossibility of submitting the substance of the hygrometer to such a degree of heat. However, I soon removed that difficulty by considering, that the degree of *heat* necessary to produce *extreme dryness*, might be applied to some substance that could bear it; and that *dryness* be transmitted to the *hygrometer*, by inclosing it with that substance in a proper vessel. The substance I chose was *potash*; and I prepared, for this and some other hygrometrical purposes, an apparatus which was made by Mess. NAIRNE and BLUNT in 1776. But a new objection stopped me again in that pursuit, and led me for some time to a very great and now almost useless labour. The degree of dryness produced by *potash* so used, could be only proportionable to the degree of *heat* that it had received; and

not conceiving yet any known limit to the intensity of *heat*, I could not expect any limit to *dryness*, nor even a fixed degree of it.

2. I remained at that point, with however a *comparable* hygrometer laboriously constructed, till I came to conceive, that *heat* must be at its *maximum* in a body, when it is *incandescent*; which opinion I have explained in my work, *Idées sur la Météorologie*. From that first idea I soon after concluded; that every *hygroscopic* substance, which could retain that property after having been brought into *incandescence*, would answer my first purpose. The following is the theory resulting from the whole of the above considerations. 1st, The *hygroscopic* substance which has the most capacity for *moisture*, and receives it the most readily, being placed in any quantity in a given space, cannot bring that space to a degree of *dryness* greater than its own; and if that degree is undetermined, it cannot afford any *fixed point* for the *hygrometer*. 2d, The *hygroscopic* substance which has the smallest capacity for *moisture*, and is the slowest in receiving it, if it is really reduced to *extreme dryness*, will have the power of producing it in a given space, provided its small capacity be compensated by a greater quantity, and its slowness by more time. 3d, Every *hygroscopic* substance, which may be brought to *white heat* without losing its property, is fit to produce *extreme dryness* in a close space. 4th, It is indifferent for that purpose, that the substance used be of the class which has a chemical *affinity* with *water*, it being sufficient that, after having been reduced to *extreme dryness*, it be still capable of receiving it from the ambient *medium*, as may every porous substance. 5th, But for the practical purpose of fixing the point of *extreme dryness* on *hygrometers*, such a substance must be chosen as, with a great

capacity for *moisture*, receives it but slowly : as by that first property it may be taken in less quantity ; and by the last, it will be less subject to acquire a sensible quantity of *moisture* in the time necessary for the operations.

3. *Pot-ash* and some other alkaline substances afforded the first of those properties, but not the last ; and I had not fixed on any substance, when, being at Birmingham in the autumn of 1782, Mr. JAMES WATT informed me, that his friend Dr. BLACK had found in *quicklime* a great capacity for *moisture*, and much slowness in retaking it : this he knew, by having kept a long time the same *lime* in a close vessel, for drying salts and capillary tubes for thermometers. These were the very properties I wanted for my purpose, which thereby I executed as soon as I came home. I made those first operations in small glass vessels, using old *lime*, which I brought again to *white heat* every time I used it. These first trials agreed with my theory in its first point ; that of producing constantly the same degree of *dryness* : as for the second, namely, whether that degree was *extreme*, it depended on other experiments.

4. Being sure then of a fixed degree of dryness, the number of experiments I undertook made me wish for a means of avoiding the frequent repetitions of bringing again my *lime* to *white heat* ; and having found one which has succeeded, I am going to describe the apparatus. The vessel, fig. 1. is of tin, 3 feet high and 1 in diameter. A glass plate *a, a, a, a*, is fixed at the top, forming a vertical section of the cylinder at 1 inch distance forwards from the axis. A woven brass-wire cage *b, b, b, b*, is fixed in the vessel through its diameter, in order to keep a space for the instruments ; for the same purpose it is open at the top, and also opposite the glass, where the dials of the instruments are to be seen. For my experiments,

ments, which required instruments of various sizes, I made that cage 18 inches high and 2 deep; but it may be much smaller for common hygrometers. The whole vessel, except that space, has been filled, through the openings *c, c*, with *quicklime* taken from the kiln, and suffered only to lose the red heat; after which the openings were covered with heaps of the same lime, which absorbed the moisture of the air entering the vessel while it was cooling, and then the openings were shut with tin plates and putty. The top of the vessel has four square openings *d, d, d, d*, correspondent to the wire cage, for the introduction of the instruments, which are hung to hooks. I use a hooked wire for putting in or taking out the instruments, to avoid bringing my fingers near the openings. These are kept shut with tin plates and putty: I never open but one at a time, which I leave open as little as possible; and to prevent the introduction of the external air in those short operations, I make them as nearly as possible at the same temperature, which being 60° of FAHRENHEIT may be obtained in every season. With these precautions, and also by moist air being lighter than dry air, there is scarcely any moisture introduced in the vessel except by the instruments themselves.

5. This application of my method afforded me a very strong confirmation of the practical fixity of the point of *dryness* produced in that manner by *lime*: for the apparatus was different from the former ones; 1st, by the quantity of the *lime*; 2d, by the *lime* having been put very hot into the vessel, while, when I used glass vessels, I had suffered it sometimes to cool down to 60°; 3d, by that *lime* being of the first calcination, instead of old *lime* brought again to white heat; and all these differences produced no sensible effect on the point of *dryness*. Since that time Mess. NAIRNE and BLUNT, Mr. HURTER, and Mr.

HAAS,

HAAS, have made apparatuses of the same kind, and I have made myself some others of different sizes and shapes; and they all produce sensibly the same degree of *dryness*.

6. The described apparatus was ready in the month of October, 1787, and I put in it one of my first *hygrometers*; which, in a few days, came to its fixed point of *dryness*, and there it has remained ever since, though I have opened the vessel above four hundred times. That degree of constancy, much beyond my expectation, has enabled me to make a variety of experiments, which else had been next to impossible: it proceeds partly from the great capacity of *quicklime* for *moisture*, which I shall determine hereafter; and partly from its slowness in receiving it; which circumstances, added to the small size of the openings, to their being at the top of the vessel, and to the care of putting in and taking out the instruments nearly at the same temperature, prevents the *lime* from acquiring any sensible degree of *moisture* during these operations.

7. I did not trust at first the apparent continuance of the same degree of *dryness* in that vessel. At the end of nine months of frequent use, I began to fear, lest the *whalebone*, of which the *standard hygrometer* is made, had been impaired; and I took it out, to try its point of *extreme moisture* (of which I shall speak hereafter;) but it came exactly to that point, and when put again into the lime-vessel it returned where it stood before. I have repeated many times that trial, with the same result; the last time was at the end of three years, when, instead of a loss of expansibility in the *whalebone*, I found a small increase, but probably accidental; it went a little farther than its point of *extreme moisture*, and came back to its constant point of *dryness*.

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8. The principles of hygrometry being now my only object, it would not be proper to enter into particulars on its practical part; but I shall here mention for once, that the steadiest *hygroscopic* substances are subject to anomalies: for instance, after an hygroscope has remained fixed in *water* for many hours, if it is taken out, suffered to dry a little, and then put again into *water*, it may sometimes happen to overpass that point. In the same manner, after an hygroscope has been long fixed in the lime-vessel, it may happen also, that in taking it out only for a quarter of an hour, and putting it in again, it will move a little farther than it was before. Again, if in taking it out of the *lime-vessel*, where it had long remained fixed, it is put into *water*, and then back into the *lime-vessel*, it may happen, that it will fix itself a little short of its former point, and never move thence, except by repeated great variations of heat; but if, when it shews that disposition, it is taken out for a short time, and put in again, it will then attain its usual point. This was the case in the last trial of my *standard*. Lastly, the same anomalies may take place at every other point of the scale of every hygroscope, only more or less according to the substances; some of which, for that reason, cannot be used for practical hygrometry.

9. Those anomalies of the steadiest *hygroscopic* substances, will probably prevent our ever having in the *hygrometer* an instrument nearly so exact as the *thermometer*; and this I was to premise, that when I mention the results of particular *hygroscopic* experiments, it may be understood, that they have only the degree of exactness that belong to their class. Luckily those anomalies are yet of no consequence for the great objects of *hygrology* and *meteorology*; the present state of *hygrometry* being sufficient to excite on those objects, questions of great importance for  
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natural philosophy. And in the mean time those anomalies are very interesting in themselves; as, from their laws, they seem to point out some modification of *cohesion*, as being the immediate cause of *elasticity* in *solids*. If I can find time to put in order a number of observations and experiments I have made in that respect, I intend to make it the subject of a Paper, in which I shall examine, from a general result of those phænomena, the comparative use of *weights* and *springs* for keeping stretched the *hygroscopic* substance in *hygrometers*.

10. After *fixity* in the degree of *dryness* produced by *lime* in the manner I have explained, the next point to be examined in respect of my theory, was, if the *nature* of the substance brought to a *white heat*, had any influence on the degree of *dryness* thereby produced; and in order to try at once the effect of a very great disparity, I chose such a *sand-stone* as is not affected by *acids*, and strikes fire from steel, before and after having been *incandescent*. The first experiment I made was with a view of finding the comparative capacities for *moisture* between that *stone* and *lime*. For that purpose I took such pieces of them as might be readily reduced to half an ounce while *incandescent*; which being done, I put them into brass cups, fitted to a scale, and I inclosed them under a glass vessel inverted over *water*. I weighed those substances from time to time; each of them continued to acquire *weight* during five weeks; at which time the *sand-stone* had gained  $\frac{1}{2 \cdot 5 \cdot 6}$  part of its original *weight*, and the *lime*  $\frac{1 \cdot 1 \cdot 0}{2 \cdot 5 \cdot 6}$ : this last was at that time all cracked and fallen in small fragments, easily reduced to powder; the *sand-stone* struck fire as before. I next prepared a cylindrical tin vessel, 10 inches in diameter and 14 inches high, with a glass top, which I filled with fragments of that *stone*, treated as the *lime*; and when it was cooled, I



put into it an *hygrometer*, whose fixed point of *dryness* has been taken in the *lime* apparatus : and in five weeks it was fixed to the same point. This is a demonstration, that the *nature* of the substance does not interfere with the *degree* of *dryness* produced, and that *incandescence* is the only cause of its *fixity*.

11. Lastly, in respect of *hygrometry*, a degree of *dryness* thus determined might have been sufficient ; but for *hygrology*, and even for natural philosophy in general, it was desirable to discover if that *fixed degree* of *dryness* was also *absolute* : and the following are the considerations which directed me in that enquiry : if *evaporation* is produced by *heat* only, and if *incandescence* is the *maximum* of *heat* ; an *hygroscopic* body, which is brought to *incandescence*, cannot contain any *evaporable* water ; and if that body has such a mass, as to be capable of absorbing all the water *evaporated* in a certain space, without acquiring any measurable *moisture*, that space may be called *absolutely dry*. Now, if an *hygroscopic* substance which is inclosed in that space, contains any sensible quantity of *evaporable* water, when *heat* increases, that substance must lose a part of its *moisture* in the *medium*, and take it back by the diminution of heat. Consequently there was a means of discovering, if *hygroscopic* substances, reduced to the above degree of *dryness*, still retain a sensible quantity of *evaporable* water ; it was that of observing their *weight*, by changes of heat : and from these previous considerations I made the following experiments.

12. I hung successively to a very sensible *beam*, shewing the changes of *weight* by an *index*, different sorts of vegetable and animal substances, the *beam* being inclosed in a glazed tin-vessel, containing a sufficient quantity of *quicklime* ; and during the operations, I produced from time to time great changes in the *temperature* of the vessel. As long as these substances

retained a sensible quantity of *evaporable* water, the increase of *heat* made them lose some *weight*, which they regained partly when the *heat* returned to the same point. But that effect diminished by degrees; and, at last, a change of  $30^{\circ}$  of *Fahrenheit* did not produce any sensible change of *weight* in those substances, though they were such as had a great capacity for *moisture*. An *hygrometer* placed near the *beam*, was then at the point taken in the *lime-vessel*. That single experiment confirms all the previous considerations from which I had expected *absolute dryness* from *incandescence*.

### *Of extreme moisture.*

13. The second proposition I had sketched in my first paper, is this “that *water*, in its liquid state, is the *only* sure immediate means of producing *extreme moisture* in *hygroscopic* bodies.”

14. *Moisture*, the nature of which we are first to determine, may be considered in three different cases.—1st, In substances which have an *affinity* with *water*; by which their *moleculæ* and those of *water* may unite, and form a new compound.—2dly, In substances which have no *affinity* with *water*, but to which *water* has a tendency to adhere; by which cause it enters their *capillary pores*.—3dly, In the *medium*, or space free from visible bodies. I have not undertaken to discover what, in the first of these cases, might properly be called *moisture*, and its *degrees*; as I foresaw great difficulties in that undertaking, which besides was unnecessary to my principal pursuit: therefore I come immediately to the second case.

15. When I wrote my work *Idées sur la Météorologie*, having not yet made some experiments I had in view to verify the opinion I entertained, that vegetable and animal substances, as  
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well as the porous mineral ones, received *water* merely by the faculty of *capillary pores*, I used sometimes the expression *hygroscopic affinity*, in treating of the *hygroscopic equilibrium*; but before that work was come out of the press, having conversed on that object with Dr. BLAGDEN, and found him inclined to that opinion, I had time and opportunity to express it, in § 276, as follows: “There are reasons to doubt, whether some  
“of the substances, which share amongst them the *water* dif-  
“feminated in a space, do not *suck* it, by a faculty similar to  
“that of *capillary tubes*, without any *chymical affinity* with  
“*water*.” That opinion will be now confirmed by the following experiments.

16. 1st. Exp. *Sugar* has an *affinity* with *water*, and no sensible one with *alcohol*: however, a lump of *sugar* will *imbibe* this last *liquid* as readily as the first. Consequently, *water* is not *imbibed* by *sugar* in consequence of their *affinity*; since *alcohol* is also *imbibed*: they both ascend in *sugar*, by the faculty of its *capillary pores*, as they do in *sand-stone* or in *sponge*. But when *water* has thus entered *sugar*, it *dissolves* it; which then is a *chymical* effect; whereas *alcohol* evaporates, and leaves the *sugar* sensibly as it was before.

17. 2d. Exp. If *water* penetrated *hygroscopic* substances of the vegetable and animal kinds, by an *affinity* with them, it would not be natural to expect, that other *liquids*, which do not shew an *affinity* with the same substances as *water*, should penetrate the above-mentioned substances. Being led by that consideration, I made two *hygroscopes* of different elastic *animal* substances; and after having marked the point where they stood in *water*, I immersed them successively in *alcohol* and in *ether*; by which *liquids* they were expanded nearly as much as by *water*, and they contracted as much in coming out of them.

In those experiments a singular phænomenon happened in both hygrosopes. The first effect of the immersion of those *animal* substances in *alcohol* (and I suppose it would have been the same with *vegetable* ones) was *contraction*, soon followed by *expansion*; and when they came out into the air, the first effect was *expansion*, soon followed by *contraction*. The cause of that reciprocal phænomenon is undoubtedly the *affinity* of *alcohol* with *water*. In the immersion, some of the *moisture* came out of the substance, to unite with the surrounding *alcohol*; by which loss of *water*, a *contraction* took place in the substance, 'till it came to imbibe the *alcohol* itself. In the immersion, some of the *moisture* of the air, uniting immediately with the *alcohol* retained by the substance, expanded it to the same degree as if it had been in *water*; after which, the *alcohol* evaporating, the substance contracted. Those phænomena did not happen with *ether*; this not uniting readily with *water*; but it expanded those substances as much as *alcohol*, and nearly as much as *water*. From those phænomena we may conclude, that the penetration of animal substances by *water*, and consequently by *moisture*, is produced, as that of *sugar*, *sand-stone*, and every other porous substance, by the faculty of *capillary pores*, without any *affinity* between them and *water*,

18. 3d. EXP. In that theory, of a mere *imbibition* of *water* by *hygroscopic* substances of the *elastic* kind, a circumstance, which seems to point out *affinity*, was to be explained; it is that of the *hygroscopic equilibrium*. In view of that object, I made the following experiments, not new in themselves, but directed to my purpose. I took some glass tubes, of different small bores, which I first bent in the shape of syphons; after which I cut them in the middle of the bent part. This was to enable me, to bring into exact communication the lower end of two tubes,

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though held in a vertical position, as an inverted syphon (Fig. 2.) The following are the experiments.—1st. EXP. When a column of the liquid had ascended in one of these tubes; if I applied to it an empty tube of the same bore, the *liquid column* divided itself equally between them.—2d. EXP. When the empty tube was of a smaller bore, that *column* rose more in it, than it sunk in the other; and the contrary happened when the empty tube was of a larger bore.—3d. EXP. When some more *liquid* was supplied to the united tubes, it rose in both, in proportion to the respective heights of the former *columns*.—4th. EXP. When a superabundant quantity of *liquid* was supplied, it rose to a *maximum* in each tube, and the heights of the columns increased in some proportion with their former heights.

19. These known facts have a clear analogy with the *hygroscopic equilibrium* in *elastic* substances.—1st. CASE. When the quantity of *liquid* common to *capillary tubes* is not sufficient for them to receive their respective *maximum*, they share it between them, and the *equilibrium* takes place, when there is, in each of them, the same ratio between its specific *capillary power* and the weight of the raised column. In the same manner; when the quantity of *water* disseminated in a space, is not sufficient, for several hygroscopic substances to receive the *maximum* of *water* which they can contain in their *pores*, they share it amongst them; and the *equilibrium* is produced, when there is in each of them the same *ratio*, between its specific *capillary power*, and the resistance of their *pores* to be more dilated.—2d. CASE. When there is a superabundant quantity of *liquid* common to some *capillary tubes*, each of them receives its *maximum*; which is determined by an *equilibrium*, between its total *capillary power*, and the *weight* of the raised *column*. In the same manner, when there is a superabundant quantity of *water* common

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to several *hygroscopic* substances, each of them receives its *maximum*; which is determined by the *equilibrium*, between their total *capillary power*, and the resistance of their *pores* to be more dilated. That final *equilibrium*, which, from its very nature, cannot be overpassed in any *elastic* substance properly used, determines the specific *capacity* of those substances for *moisture*.

20. *Moisture* then, considered in *porous* bodies not *soluble* by *water*, may be defined, “A quantity of *water*, which is “*invisibly* contained in their *pores*; without any other connection with their substance, than that which it has with the “*glass* of the *capillary tubes* into which it has ascended.”

21. We may see now whence proceeds the *hygroscopic equilibrium* between *elastic* substances inclosed in a space, either filled with *air* or deprived of it. In this explanation it is unnecessary to determine, how *water* is *invisibly* disseminated in spaces free from visible bodies, therefore I shall not enter here into this subject; that dissemination is a fact admitted in every hypothesis, consequently the *medium* is only to be considered as the stock and standard of *moisture*. By the cause, whatever, of *evaporation*, *hygroscopic* substances lose or gain *water* in the *medium*, according to its degree of *moisture*, till they are in *equilibrium* with it; which implies the *equilibrium* amongst themselves according to the laws resulting from their own nature.

22. Now *moisture*, in a general sense, will appear to be, “a “quantity of *invisible water*, either *evaporable*, or *evaporated*.” And from that definition, the *maximum* of *moisture* will exist, when, “every circumstance remaining the same, no more “*water* can be admitted in a space, without becoming *visible*; “on *solid* bodies, by their surface being *wet*; and in the *medium*,

“by a spontaneous *precipitation of water*.” Lastly, as immersing solid *hygroscopic* bodies in *water*, or exposing them in a *medium* where there is an actual *precipitation of water* (as in a *fog*) is an effectual means of furnishing their *pores* with the whole quantity of *water* they can *imbibe*; it is evidently a *sure* means of producing *extreme moisture* in them: and this point cannot be overpassed, neither in *water*, nor in *fog*, since it depends upon the *resistance* of the *pores* to further dilatation by the mere introduction of *water*; but it must be attained in an *hygrometer*, to fix its point of *extreme moisture*.

23. When formerly I had fixed upon that method for procuring to my first *hygrometer* a true point of *extreme moisture*, it occurred to me, that the *temperature of water* might influence sensibly the expansion of its *ivory* tube; and in order to discover if it was so, I made some experiments, related in §§ 104 &c. of my former paper, the result of which was, that the *temperature of water* had a sensible effect on the expansion of *ivory*. But soon after I distrusted some modifications of that complicated *hygrometer*; and especially this particular result.

24. I then changed that first method; which consisted in measuring the changes of capacity of hollow cylinders, into that of measuring the changes in length of *hygroscopic* substances; and for some preliminary experiments on many of them, I made particular *frames*, in which, by a combination of *glass* and *brass*, the effects of *heat* on those materials compensated each other; by which means the *indices* of those instruments were only affected by the modifications of the *hygroscopic* substances tried in them. I have mentioned these *frames* in a paper on *hygrometry*, printed in the *Phil. Transf.* for 1778. With these *frames* I first tried *ivory*, in *water* of different *temperatures*;  
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and I found a very little difference in its *expansion*, comparatively with what had appeared from my first experiment. Then, continuing the same trial on various substances, I found the effect of different *temperatures* of *water* very small in general; and even in some substances, as *deal* taken lengthwise, and *bemp*, I could not ascertain any.

25. These experiments led me to think, that the small variations produced by *heat* in hygroscopic substances dipt in *water*, were not *hygroscopic* modifications, but the mere effects of *heat*, by the cessation of all *hygroscopic* modifications; these having then attained their *maximum*: which is a discrimination of effects, that I had vainly attempted to produce by other means. When afterwards I had found the method of producing *extreme dryness*, I made a lime apparatus, for the purpose of repeating in it the same experiments with my *compound frames*; and I found that theory confirmed, by the effects of *heat* in that apparatus being nearly the same, on the same substances, as when they were in *water*.

26. From the whole of the foregoing experiments there cannot remain any doubt, that *water*, in its liquid state, is a *sure* means of fixing the point of *extreme moisture* on *hygrometers*. Particularly, in respect of *elastic* substances, as *ivory*, *quill*, *whalebone*, all sorts of *wood*, and a number of others which I have tried, the last experiments in *water* of different *temperatures*, afford an immediate proof, that their faculty of *sucking* water has a fixed limit, proceeding from a final *resistance* of their *pores*, to be more dilated by the introduction of *water*. Consequently, their *utmost expansion* is a true sign, that *moisture* is *extreme* in them; which point cannot be exceeded. But my proposition extended farther: I had said, that *water* was the *only* certain means of obtaining immediately the point of  
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extreme *moisture* on *hygrometers*; and this is a most important question, both of *hygrometry* and *hygrology*, which remains to be examined.

*On the maximum of evaporation, and its correspondence with the maximum of moisture in a medium.*

27. Since *moisture* consists in *invisible water*, an *excess* of *water* is the only immediate means of ascertaining that the *maximum* exists; as, if a reservoir is above our reach, the only means of knowing if it be *full* of water, is when it overflows. From that principle, a *fog* gives the point of *extreme moisture* on *hygrometers*, like *water* itself; because it covers very soon the *hygroscopic* substance with a coating of *water*: sometimes even it expands it a little more than an immediate application of *water*; but this belongs to an object that I have waved before, as relating to some modifications of *elastic solids* (§ 8.). No other means then but an *excess* of *water* over the surface of the *hygroscopic* substance of the *hygrometer*, can ascertain that it is arrived at its point of *extreme moisture*; and the first immediate demonstration I shall give of it, will be afforded by *dew*; a very uncertain, though apparently certain sign of *extreme moisture* in the *air*. We say, that there is *dew*, when some solids exposed in the open air in a clear evening are *wet*; but if that was the effect of a *precipitation* of *water* happening in the *air*, all the solids thus exposed would be *wet*; which is far from being the case; consequently, that phenomenon must proceed from some particular causes, by which, though no *water* is yet disposed to abandon the *medium*, it gathers on some particular solids. It is very long since the phenomena of *dew* have perplexed natural philosophers; and they were the first

which I studied in the beginning of my researches in meteorology; but all that I concluded from my experiments and observations was, that we could not understand those phænomena without first having a sure *hygrometer*. This is the reason why, soon after I had made my first *hygrometer*, I exposed it in the open air in the country, suspended very little above the grass, from the morning of a fine day to the time of *dew* in the evening; the *grass* grew *wet*, and the *hygrometer* remained at a great distance from the point which had been fixed in *water*. I have related that experiment in § 91, of my first Paper.

28. When I had made hygrosopes of various sorts of *slips*; for instance, of different *woods* and of *whalebone*, cut across the fibres; of *ivory* and *horn*, reduced first into thin tubes, and then cut in screw; and of *quills*, by cutting also in screw their barrels; I repeated, with those instruments, my observations on *dew*; and to give a short, but determinate idea of the phænomena I observed, I shall reduce them to some general cases, as indicated by one only of those *hygrosopes*, that of *quill*, which, like all the others, is divided into 100 parts, from *extreme dryness* to *extreme moisture*. These hygrosopes were suspended in the open air, three feet above a grass-plot in the country. 1st CASE. When a clear and calm evening succeeds to a clear and warm day, the *grass* frequently grows *wet*, though the above *hygroscope* stands many hours, and sometimes the whole night, between 50 and 55. 2d CASE. If the *dew* increases, so that taller herbaceous *plants* and *shrubs* grow *wet* in succession, the *hygroscope* moves more and more towards *moisture*; and when it is come to about 80, plates of *glass* and *oil-paint* also grow *wet*; but at that period, neither *metallie* plates, exposed like the glass ones, nor some *shrubs* and *trees*, are *wet*; and this also may last whole nights. 3d Case. If the  
*dew*

*dew* proceeds to its *maximum*, the *hygroscope* moves from 80 to 100 (and sometimes a little farther, § 27.). Then we have also a certain proof that *extreme moisture* exists in the air; for every solid body exposed to it is *wet*. But it is only at that moment that we can depend on *extreme moisture* existing; for, if in the other described stages of the phenomenon, the appearance of *water* on the surface of some solids had proceeded from a spontaneous precipitation in the air, all the other solids ought to have been *wet*; but they only become *wet* in a certain succession, and in the mean time the slip of *quill*, and all the other above-mentioned *hygrosopes*, move more and more towards their point 100, in sign of *moisture* increasing in the air. Consequently (as I had concluded from my first observations), instead of having in *dew* an *hygroscopic* standard for the *hygrometer*, we have in its phenomena many circumstances which will only be explained with the assistance of that instrument.

29. Some previous observations had also warned me against the general idea, that *moisture* was to be *extreme* in the air, when there was a sufficient quantity of *water* in the space, even though that *air* might be supposed to be filled with *evaporated* water to its *maximum*; and the doubts I entertained in that respect were the cause of the difficulties I expressed in the beginning of my first Paper, which I only got over when I thought of *water* itself, to obtain the point of *extreme moisture* on my *hygrometer*. This was also the reason why, as soon as my first *hygrometer* was finished, I placed it in a cellar, the walls and ground of which were *wet*, and where it continued two months, without ever attaining its point of *extreme moisture*. I have related that experiment in § 54. *et seq.* of my first Paper.

30. When also I had the *hygrosopes* mentioned above in the observations on *dew*, I undertook a very long course of various sorts of experiments on that important point of *hy-*

gology, of which however I shall only give here the general and constant results, as furnished by those hygrosopes. “The  
 “ *maximum of evaporation* in a mass of inclosed air is far from  
 “ being identical with the *maximum of moisture*; this being  
 “ dependent also, even to a very great degree, on the *tempera-*  
 “ *ture of the space*, supposed to be the same, or nearly so, as that  
 “ of the *water which evaporates* in it. *Moisture* may arrive to  
 “ its *extreme* in an inclosed air, if that common *temperature*  
 “ is near *freezing point*; but it becomes less and less, even to a  
 “ very dry state, as that *temperature* rises, though the *product*  
 “ of *evaporation*, thereby increasing, continues to be at its dif-  
 “ ferent *maxima*, correspondent to the different *temperatures*.”  
 This is a very important proposition in *hygology*; which, from  
 my experiments, would not be subject to any objection, if there  
 were no other *hygrosopes* than those I have mentioned above, of  
 which I have thirteen different species; but there is another  
 class of such instruments, from which some doubt might first  
 arise; and I come now to that point.

*On two distinct classes of hygrosopes.*

31. As I shall now frequently speak of *slips* and *threads*,  
 which constitute those two *classes* of *hygrosopes*, I must first  
 explain what I mean by those words. The *slips* compose the  
 class of *hygrosopes* used in the above experiments; they consist  
 of very thin and narrow *laminae* cut *across* the fibres of vege-  
 table or animal substances, either in their natural or artificial  
 breadths (as *boards*), or by reducing natural or artificial thin  
 tubes of them into *belices*. By *threads* I mean the same kinds  
 of substances taken *lengthwise*, either from their being natu-  
 rally in thin threads, or by reducing them to that state, in  
 3 tearing

tearing from them thin *fasciculi* of *fibres*; which operation is easy in some, as *hemp*, *whalebone*, and *gut*, but very difficult in others, as *quill* and some sorts of *wood*.

32. The first *hygroscopes* of the class of *threads*, which I observed comparatively to the class of *slips*, were of *hemp*, *gut*, *whalebone*, and some *woods*, and they exhibited a phenomenon which at first I could not understand: when they were exposed with the *slips* in *damp* air; as, for instance, in open air during the second period of *dew* above determined, or in a glass vessel inverted over *water*; the *threads* had only very small motions backwards and forwards round their point determined in *water*, while the *slips* had considerable motions within that point, without coming near it, if the *temperature* was sensibly above *freezing point*. Thence arises the objection against the general proposition above stated: for these two classes of *hygroscopes* contradicting one another on the changes of *moisture*, nothing could be asserted in that respect, till there were sufficient reasons to exclude one of those classes of informers, and to trust the other class.

33. Proceeding to multiply the species of those two classes of *hygroscopes*, I found always the same fundamental *march* in *slips*, all of them constantly moving in the same direction; but in multiplying the species of *threads*, I found such variety between them, that in their own class they created distrust; some of them, as of *deal*, *aloes pitta*, *liber of lime tree*, *quill*, and thin *stems of gramen*, in coming out of *water*, increased in *length*: they went farther that way, to a certain point, as the air was dryer: they *retrograded* then with accelerated steps, when *dryness* increased, thereby returning to the point where they had stood in *water*; and they continued to move in the same *retrograde* way, with great acceleration, by a still in-

creasing

creasing *dryness*. Moreover, they did not follow one another in those motions contradictory to the evident *march* of *moisture*: each of them changed direction at different periods, thereby often contradicting also each other, while the *slips* constantly agreed together in the direction of their motion, and also with all the other symptoms of *moisture*. From these comparative phænomena I first concluded; that the motions of the same kind, which I had observed in the first-mentioned *threads*, were also *anomalies*, proceeding, only to a smaller degree, from a cause of the same nature as that of these last. After which, similar symptoms, which I had formerly observed in the *water* thermometer near the *freezing point*, made me first conclude, from a general analogy; that the perceivable modifications of the *threads*, were the compound effects of two contrary operations of *moisture* which followed different laws.

34. Another phænomenon led me soon after to a more determined theory in respect of those two opposite effects of *moisture* on *threads*. I have said above, that *bemp* and *gut* have only a very little *retrogradation*; their greatest difference from the *slips* consisting in their being *stationary*, while the *slips* have still great motions. But when these same *threads* are *twisted*, they acquire a very sensible *elongation* beyond their point of *extreme moisture* succeeded by *retrogradation*. From several trials I have made in *twisting* these *threads* more and more, I do not consider as impossible, if some difficulties, which I only could obviate in part, were completely prevented, that they might be brought to such a state, as to have their point of *extreme dryness* coincide with that of *extreme moisture*; by which means, in the progress of *moisture* from one *extreme* to the other, they would move first in one direction with decreasing steps, then in the opposite direction by increasing steps; the whole, however,

with great irregularities. Here then we see two *opposite* effects of *moisture*; one which *lengthens* the *fibres*; the other which, by *swelling* the *twisted* strings, *shortens* them; and we see those effects follow different *laws*, from which is produced a *retrogradation* that we may change *ad libitum*.

35. Now, the texture of animal and vegetable *fibrous* substances must be a sort of *reticle*, which exists in those which are naturally in thin *threads*, and in the most minute *fasciculi* that we can separate from a mass; and we see it in the last case, for in subdividing those *fasciculi*, there are always *fibres* breaking in the points where they were anastomosed with others; consequently, the primary *fibres* of those substances form between them *mesbes* similar to those of a *net*; and those *mesbes*, which are *widened* by the introduction of *water*, must produce in the *threads* the same effect as the *twist* in the above *strings*.

36. If then *moisture*, in acting on vegetable and animal *threads*, natural and artificial, produces on their *length* two opposite *effects*; one of which, small at first but increasing gradually, *compensates* at some period the other which is first visible, and *surpasses* it afterwards, sooner or later, according to the nature of the *threads*; it is evident, that they cannot be proper for the *hygrometer*; since, from the indication of some of them it might sometimes be concluded, that *moisture* changes in one sense, while it really changes in the contrary sense; and from some others, that *moisture* is *extreme*, long before it is really so. As for the *slips*; since *moisture* has only one effect on their *length*, that of *widening* more or less the *mesbes* of the *cross fibres*, I concluded; that all their *hygroscopic* indications, in every part of their scale, were *true* in respect of *increase* and *decrease* of *moisture*; and that consequently, that class of *hygroscopes* might be depended upon on that important point.

point. As for the exact *ratio* between the *indications* of those last *hygroscopes*, and the *changes* of *moisture*, that was to be the object of a particular inquiry, to which I now come.

*Of the scale of the hygrometer between the two fixed points.*

37. The long attention I had formerly given to the comparative *expansions* by *heat* of various kinds of *liquids* and *solids*, made me expect the variety I afterwards found in the modifications of *hygroscopic* substances by *moisture*; therefore, as I expressed it in § 2. of my first Paper, my view was only at that time, to find some means of producing a steady *comparable* hygrometer; but afterwards I pointed out, in § 72, a means which had occurred to me, for attempting to find the *ratio* between the *expansions* of some determined *hygroscopic* substance, and the correspondent *increases* of *moisture*; which was, to compare the first with the correspondent changes of *weight*, of the same or of any other substance; an idea which I did not then much scrutinize, not yet thinking of its execution.

38. From what I have said above, I did not want any other motive of choice between the *slips* and the *threads* than their comparative *marches*; but though the *slips* agree always in the *direction* of their motions, there are differences in the progression of their comparative steps; and that difference led me to examine more attentively the above means of finding which of those *marches* agreed best with that of *moisture*. The result of that examination was distrust, at least in respect of an immediate decisive means. With the view of rendering the changes of *weight* more easily measurable, I had first thought of some substance possessed of a strong *affinity* with *water*; but on con-  
sidering



sidering the *hygroscopic* phænomena of those substances, it appeared to me, that their changes of *weight* could not be proportional to the degrees of *moisture* in the *medium*; and that even the sense of the word *moisture* applied to them was very difficult to determine (§ 14.). As for the substance of the *hygrometer* itself, I did not find any reason to think, that its changes of *weight* could be more proportional to *moisture* than its degrees of *expansion*; since on these last depended in part the quantity of *water* that could be admitted into its pores at each degree of *moisture* in the *air*.

39. Being thus disappointed in my first scheme, I thought of a more direct method, which was to act on *moisture* itself, by first producing, in a glass vessel containing an *hygroscope*, as much *dryness* as I could conceive possible at that time, and then introducing into it successive equal quantities of *water*, for which I had found a sure means without opening the vessel. But then again some previous experiments destroyed my confidence in that method; having found,—1st, That the evaporated *water* had a tendency to deposit itself against the *glass* by the smallest difference between the inside and outside *temperatures*, even to the degree of becoming visible on some part of the vessel, long before I had any reason to expect *extreme moisture*.—2d, That by the common *temperature* of my room, the *hygroscope* in the vessel remained always at a considerable distance from its point of *extreme moisture*, though the bottom of the vessel was covered with *water*; and that it varied with the *temperature*; which could not have happened if *moisture* had been *extreme*.—3d, That when I endeavoured to increase *moisture* in the vessel by cooling it, I produced very often the contrary effect, at the same time that a quantity of the disseminated *water* gathered over the glass.

40. As in those trials the contradiction between the *marches* of the *slips* and the *threads* was very evident, I was the more disappointed to find, that the uncertainty of the real degrees of *moisture* increased at that very period: for instance, setting out from some *dry* point, and *moisture* increasing, the *march* of a *thread* of *whalebone* was evidently in a *decreasing* progression, comparatively to that of a *slip* of the same substance; and when at last there was a superfluous quantity of *water* in the vessel, and the *temperature* was made to change, the *thread* was almost *stationary*, while the *slip* had considerable motions, often contrary to the small motions of the other; while it was at that very period, that the real degrees of *moisture* in the vessel could not have been ascertained but by an already settled *hygrometer*. I had no doubt that those anomalies were to be attributed to the *thread* of *whalebone*, not only because of the excess of the same modification in other *threads*; but also considering the analogy between the comparative *marches* of the two kinds of *hygroscopes*, and those of the *thermometers*, of *water* and *quicksilver*: but as this was a very important object for natural philosophy, I would not decide it from those first appearances; and that consideration led me to a very great number of various sorts of experiments, which I made in view of multiplying at least the indirect facts with which my theory might be compared. However, as at last I found a more direct means of verification, I shall only mention that last class of experiments.

*Experiments on the comparative changes of weight and dimensions of some hygroscopic substances.*

41. I have said above, that I could not find any solid reason to consider the changes in *weight* of a substance, as being more proportional, than its changes in *dimensions*, to the correspondent changes of *moisture* in the *medium*; which doubt had prevented me from undertaking that course of experiments. But it occurred to me at last, that if my theory on the comparative *marches* of the slips and the threads were true, it might be rendered certain by comparing those marches with the increase of *weight* of the same substance: for instance, taking a *slip* and a *thread* of *deal*, and having some *deal* hung to a scale, I was to find, that while the *slip* continues to lengthen, and the thread to shorten, the substances continue to receive *water*; and, in general, that the *march* of *slips*, in every part of their scale on which the experiment may be regular, was more proportional, than that of *threads*, to the correspondent changes in *weight* of every *hygroscopic* substance of the *elastic* kind.

42. This having struck me as a sure means of deciding the question, I set immediately to work; and since that time I have made a great number of experiments of that kind. They were not at first very accurate; but successively I have mended both the instruments and the apparatus; and, after having settled every part that I looked upon as essential, and made in consequence a new apparatus and new instruments, I have begun a regular course of experiments, of which I shall give here the first results.

43. The apparatus consists in two tin vessels; the first of which, and the most used, is  $16\frac{1}{2}$  inches high,  $15\frac{1}{2}$  wide, and 5 deep. The front of this vessel is a plate of glass, and the back a tin-plate *slider*, which, being taken off, leaves that side

of the vessel quite open. The second vessel has the same dimensions as the first; but its back is foldered, and its front is of woven brass wire. This vessel may be applied to the back of the former, in such manner as to make of both one single vessel, which, when the *slider* of the fore-part is taken off, is only divided by a vertical partition of woven brass wire. The use of that second vessel is to produce *extreme dryness* in the other; for which purpose it is filled with large pieces of *quick-lime* taken from the kiln. When that vessel is not used, it is kept in a tin box which it fills intirely; and when it is in, as well as while it is out for use, that box is kept shut with putty, by which means the same *lime* may serve many times in the following manner.

44. When I want to produce *extreme dryness* in the first vessel, I apply to it the second, fastened with hooks; I then pull out the *slider* of the first, and stop with putty the chinks between them. When that first operation is completed, I put again the *slider* to the fore-vessel, and take off the other. In this last operation, some *moisture* might be introduced through the chinks of the slider, before they are again stopped with putty; especially as the destruction of *moisture* in the vessel has made room for more *air* to come in; but I prevent it by making first the apparatus sensibly warmer than it was when I put on the lime-vessel; by which means, in the little time employed for the operation, the motion of the air is from the inside to the outside, which prevents all access of *moisture* in the vessel.

45. To that first operation succeeds that of a gradual introduction of *moisture* into the apparatus. It would have been useless to apply to that process the means I had imagined, for spreading successive equal quantities of water in a close vessel; for the evaporated *water* depositing itself in part, more or less, on every surface, and the surfaces, especially of *glass*, being  
very

very much multiplied in that apparatus when filled with instruments, no real correspondence could be expected between the quantity of evaporated *water* and the motion of the instruments; therefore I again gave up the view of determining that *ratio*, and proceeded in the following manner. At the bottom of the apparatus, on one of its sides, there is an opening,  $\frac{1}{2}$  an inch high, and  $2\frac{1}{2}$  inches broad, which usually is kept shut with a tin plate and putty. The taking off, only for a moment, that tin plate, is the first operation by which I introduce *moisture* into the apparatus; which being then reduced to the *temperature* at which I make all my observations, namely,  $60^{\circ}$  of FAHRENHEIT, permits the external air to enter the vessel, and come to an *equilibrium* with it at the same temperature. By keeping off the tin plate longer and longer, I admit new quantities of *moisture* into the vessel; and when that means is become ineffectual, I introduce through the same opening, a brass frame, which extends under all the instruments, on which is stretched a cloth that I *wet* by degrees more and more, as long as it may produce some effect at the above *temperature*. In order to have that *temperature* when I want it, I make those experiments in a season when I may have fire in a stove at a proper distance from the apparatus. The time when that equal *temperature* is necessary in many respects, is that of the observations, which I make twelve hours after each new introduction of *moisture*.

46. The instruments I place in that apparatus are of two kinds; the first of which are *beams*, made on the principle used by Mr. JOHN COVENTRY in his *paper hygrometer*, which principle I have found of great use; for, with *beams* of that sort, as delicate as mine are, if the total change of weight in an experiment is not above 1 grain,  $\frac{1}{1000}$  part of it may be distinctly observed on a weight from 3 or 4 to 20 grains; but in my experiments,

in which the total variation was from 5 to 6 grains, the observable part was only  $\frac{1}{16}$  part of a grain. These two *beams* are placed on the same line through the middle of the depth of the vessel, and their indices move in that plane; their motions are in an opposite sense by the same changes of weight, because I wanted the two substances suspended to the *beams*, to hang near one another in the middle of the vessel. The other instruments are *frames*, in which an *index* is moved by the variations in *length* of a very thin *slip* or *thread*. These frames are placed before and behind the *beams*.

47. In the first experiment that I made with that apparatus, the substances suspended to the *beams* were *deal* and *quill*, reduced to very thin *shavings*, which were stretched edgewise in thin brass-wire frames. The weight of each kind of these *shavings* was 12 grains, at a certain degree of the thermometer and of my hygrometer. The other *hygroscopes* were *slips* and *threads* of the same substances as the *shavings*, and also of *whalebone*. These six last instruments have their point of *extreme dryness* taken in my *lime-vessel*, and their point of *extreme moisture* in *water*; the interval between these points is divided into 100 parts; and on the *scales* of the *threads* the *degrees* are prolonged beyond this last point. The *hygroscopic* scale of the *shavings* could not be fixed before the operation; therefore the scales of the beams served only to indicate the comparative motions of the index; but afterwards, taking for 0, the point where the *index* stood by *extreme dryness*, and for 100, the point of *extreme moisture*, which I shall explain, the interval between these two points became a *modulum* by which I have also reduced into 100 parts of the *whole* the changes observed in the *weight* of the *shavings*. I shall not give here the absolute quantities, either of the changes of *weight*, or  
of

of those in *length* of the other *hygroscopes*, having not had time to make the necessary calculations, of which however I have the *data*.

48. The necessary time for a complete diffusion of the newly introduced *moisture* in the vessel renders it impossible to proceed, in that introduction, by regular steps. The method I use is, to observe the motion of my usual *hygrometer*, which is a *slip* of *whalebone*, and to remove the cause of increase of *moisture* before it has moved 5 degrees. In that manner the steps of the increasing *moisture* have been in general less than 5 degrees of that instrument; but, by interpolation, I have reduced them to what they would have been if the same instrument had been moved successively 5 degrees.

49. I have said before, that when the *maximum* of *evaporation* is produced in a close vessel by a *temperature* sensibly above the *freezing point*, there is no regularity to be expected in any farther attempt to increase *moisture*; the disseminated *water* being then abundant, the smallest difference of temperature between different parts of the apparatus makes it deposit itself on some surface, and pass from one to the other (§ 39.); which circumstance is also mentioned by Professor PICTET of Geneva, in his late work, *Essais de Physique*. For that reason, when the *evaporated water* in the vessel was near its *maximum*, my last operation was, to put in again the *wet* cloth, while I kept the *temperature* at  $60^{\circ}$ , and to take it out when the *indices* of the *beams* were fixed. My usual *hygrometer* was then at  $87^{\circ}$ ; and as it had still 13 *degrees* to move towards its point of *extreme moisture*, and all the others in proportion of their known *marches*, I have added to the observed increases of *weight* in the *shavings*, a quantity proportional to their former *marches*

2

comparatively

comparatively to that of their respective *slips*; and thus are completed their *hygroscopic scales*. Repeated trials have shewn me, that the *weight* of the *shavings* increases as long as their *slips* increase in *length*; but as there is no regularity in their comparative *marches* at that period of *moisture* in a vessel, nor any possibility of making these experiments in open air, because of the *beams*; the addition mentioned, which forms the three last *terms* of the columns of the *shavings* in the following table, is to be considered only as having determined the *modulum* of the *observed terms*; since it has not changed the *ratio* between them, nor consequently the correspondent *marches* of *weight* and *length* so far; which were the only object of the experiment.

50. Before I come to the general result of that experiment, I shall place here a comparative view of the two kinds of phænomena, which, by their analogy, led me first to the theory I have exposed: I mean the comparative *marches* of the *slip* and *thread* of *whalebone* on one side, and those of the *thermoscopes* of *quicksilver* and *water* on the other side. In this table the correspondent *terms* of the *hygroscopes*, from 0 of both to 85 of the *slip*, have been observed in the above experiment; the four following are the results of observations in time of increasing *dew*. The correspondent *terms* of the *thermoscopes* are deduced from the *table* of their comparative *expansions* which I have given in § 418. *m.* of my work, *Rech. sur les Mod. de l'Atmosphère*; from which *table* this only differs, 1st, by a change of the *modulum*, in the *ratio* of 80 to 100; 2d, by an *inversion*, which brings the *terms* of this table to express comparative *condensations* of the two *liquids*.



HYGROSCOPES.			THERMOSCOPES.		
	Whalebone Slip.	Whalebone Thread.		Quickfilver.	Water.
<i>Extreme</i>	0	0,0 <i>dryness.</i>	<i>Boiling</i>	0	0,0 <i>point.</i>
	5	12,1		5	9,3
	10	30,1		10	18,3
	15	41,1		15	26,3
	20	51,1		20	35,0
	25	59,1		25	42,7
	30	65,6		30	49,2
	35	71,1		35	56,7
	40	76,5		40	63,1
	45	81,8		45	69,0
	50	85,8		50	74,5
	55	88,8		55	79,1
	60	91,3		60	83,8
	65	93,3		65	87,9
	70	95,6		70	91,8
	75	97,6		75	95,0
	80	98,6		80	97,5
	85	99,6		85	98,9
	90	100,1		90	99,9
	95	100,5		95	100,5
<i>Extreme</i>	100	100. <i>moisture.</i>	<i>Freezing</i>	100	100. <i>point.</i>

51. The first part of that table shews, the great steps of the *bread* of *whalebone*, comparatively with those of the *slip* of the same substance, at the beginning of their correspondent increase in *length*; but the *thread* relents by degrees, and a nearly *stationary* state succeeds, in which, while this *hygroscope* moves first 1,9 *degree* from 98,6 to 100,5, then *retrogrades* 0,5

*degree* to come to its point of *extreme moisture*; the *slip*, continuing to move in the former direction, goes over 20 *degrees*. The phænomena are the same in the part of the table relating to the *thermoscopes*; that of *water* proceeds also at first by great steps, comparatively to that of *quicksilver*; after which it relents, and while it moves only 3 *degrees*, and *retrogrades* 0,5 *degree* to come to the *freezing point*, the *quicksilver* one, continuing to move in the same direction, goes over 20 *degrees*. It was from that phænomenon of the *water thermoscope* that I concluded formerly, that there was in its *march* a *stationary* state, during which the *beat* decreased nearly as indicated by the *quicksilver* one; and that conclusion was afterwards confirmed by direct experiments. From that ascertained fact, I was led to conclude, with respect to moisture, that there was also a *stationary* state in the *march* of hygroscopic *threads*, even in those which had the smallest *retrogradation*, as that of *whalebone*; and this theory will be confirmed by the results of the above-described experiment.

52. The following table contains those results, namely, the correspondent *marches* of all the mentioned *hygrosopes*; the *shavings* increasing in *weight*, and the *slips* and *threads* in *length*. The 3 last comparative *terms* do not result from that particular experiment; for the *shavings* (as I have said above) they are concluded from the former comparative steps; for the other instruments, they have been obtained by observations in the open *damp* air.

	WHALEBONE.			QUILL.			DEAL.	
	Slip.	Thread.	Shavings.	Slip.	Thread.	Shavings.	Slip.	Thr.
Extr. dryn.	0	0,0	0,0	0,0	0,0	0,0	0,0	0,0
	5	12,1	7,0	4,8	40,0	6,2	5,4	42,0
	10	30,1	13,0	9,7	72,0	9,4	11,2	69,4
	15	41,1	20,0	14,4	85,0	15,6	16,5	94,8
	20	51,1	26,0	19,2	95,0	22,6	21,9	107,0
	25	59,1	31,0	23,9	101,0	27,0	27,2	113,6
	30	65,6	36,0	28,5	105,0	33,2	32,7	118,6
	35	71,1	42,0	33,3	107,0	36,0	38,3	122,6
	40	76,5	43,8	38,3	102,0	41,2	43,7	120,6
	45	81,8	48,3	42,9	104,0	46,7	49,2	123,6
	50	85,8	52,3	47,4	107,0	49,7	54,6	126,6
	55	88,8	56,5	52,4	103,0	56,1	59,9	119,7
	60	91,3	60,5	56,9	105,0	59,9	64,9	122,7
	65	93,3	64,4	61,9	106,0	63,7	69,7	119,7
	70	95,6	69,4	67,2	108,0	67,1	74,5	117,6
	75	97,6	74,0	72,2	107,0	73,4	79,0	115,6
	80	98,6	78,0	77,8	106,0	78,1	83,5	112,6
	85	99,6	84,0	82,8	105,0	83,8	87,5	110,0
	90	100,1	* 88,0	88,2	103,6	* 88,8	92,0	107,0
	95	100,5	* 94,0	94,0	102,0	* 93,8	96,0	103,6
Extr. moist.	100	100.	* 100.	100.	100.	* 100.	100.	100.

From the 18 first *terms* of that table, which are the immediate results of the experiment, we are now to examine my opinion, that the *lengthening* of the *slips* of *whalebone*, *quill*, and *deal*, beyond these *terms* is a sure sign that, till they have attained their point 100, *moisture* continues to increase in the *medium* where they are placed.

53. There could not be any doubt on that proposition, if it were not for some *threads* similar to that of *whalebone*; which *threads*, having sensibly attained their utmost *length* at the period when the experiment was stopped, seem to indicate, that

*moisture* is then at its *maximum*. But if the *lengthening* of *hygroscopic threads* in general, is the compound effect of two opposite causes which follow different laws; it may be that, in some *threads*, those causes happen to compensate each other at that very period; by which means they are *stationary*, though *moisture* continues to increase. This was my opinion, and the above experiments were undertaken to verify it, first, by comparing the *march* of the *slips* with the increase in *weight* of their own substance; secondly, by comparing the *marches* of different kinds of *threads* with each other, and also with the increase in weight of their substance: and from that now we are to examine the above proposition.

54. In respect of the *slips* my theory is, that as *moisture* cannot act on their length but by widening the *meshes* of their transversal *fibres*, they cannot go on *lengthening* but by imbibing more and more *moisture*, from its increase in the air; and this we see to be the case, by comparing the *marches* of the three kinds of *slips* with the correspondent increases in *weight* of the *quill* and *deal* shavings, during the whole progress of the experiment. There are differences in those *marches* as I expected (§ 37.); but they are not such as to give the smallest reason to suspect, that afterwards, during the period of the three last *terms* of the table, in which we have no correspondent observations of increases of *weight* in the *shavings* of *deal* and *quill*, the same law does not take place as in the 17 antecedent *terms*. If the experiment was only made with one kind of *slip*, it might be objected, that though that *slip* lengthens regularly during the whole increase of *moisture* from its *minimum* to its supposed *maximum*, it is not impossible but that immediately after, by some peculiarity of its nature, it will *lengthen*, without any farther increase of *moisture* in the  
medium.

*medium*. But that surmise cannot be admitted when the *slips* of such dissimilar substances as *whalebone*, *quill*, and *deal*, sensibly agree in their *motions* at that period, and when a number of other *slips* of the vegetable and animal kinds follow also the same general *march*.

55. In respect of the *threads*, which are the only cause of the above doubt, my theory, which removes it, is also confirmed by that experiment. That cause of doubt is exemplified in the table by the *thread* of *whalebone*, which has almost no motion, while its *slip* moves from 85 to 100. At that period of *moisture*, no regularity can be expected from experiments made in *close vessels*; by which circumstance, not having the correspondent observations of *weights*, it cannot be demonstrated by immediate experiments, that the *thread* of *whalebone* is then *stationary* from its nature; but the *threads* of *quill* and *deal*, which are in that state during the regular course of the experiment, will guide us in that enquiry. The *thread* of *quill* is *stationary* during that great part of the observed increase of *moisture*, by which the *thread* of *whalebone* moves from 71 to 97,6: the *thread* of *deal* is also *stationary*, while the same *thread* moves from 71 to 91,3. They both afterwards *retrograde*; the *quill* from 107 to 100, and the *deal* from 122,6 also to 100; and it is during the latter part of that *retrogradation*, correspondent to a continued *direct* motion of the *slips*, that the *thread* of *whalebone*, and some more of its class, after a very *decreasing* march comparatively with all the *slips*, are at last *stationary*, and then a little *retrograde*. In that *stationary* state of the *threads*, while *moisture* proceeds in the same direction, they move backwards and forwards, more or less, according to the duration of that state, and to the quantity of the *retrogradation*. This may be seen by the table, in  
respect

respect of the *thread* of *deal* and *quill*; and I have observed it constantly, in a smaller proportion, in all the *threads* which have their *stationary* state at the last period of *moisture*, with this particular circumstance in all those motions backwards and forwards, that they are never the same in two different experiments. That symptom already points out a complication of causes; but we shall soon see a more distinct proof of their existence.

56. My theory on the *march* of hygroscopic *threads* is founded on this general principle, that a *retrograde* effect, how small soever it may be, if it is not produced by a correspondent change in the cause itself, is preceded by a *stationary* state, during which, and the *retrogradation*, the intensity of the cause continues to *increase*: and this also is exemplified in the experiment. The *stationary* state of the *thread* of *deal* begins, when its *shavings* have only imbibed from the air a quantity of *water* = 36; it is still at the same point when the quantity of imbibed *water* has increased to 59,9; and in the part of its *retrogradation*, which is still contained in the regular course of the experiment, that quantity of imbibed *water* increases to 88,8. The same, with only some differences in the degree, is seen in the *thread* of *quill*; therefore, as we see also some, though a very small, *retrogradation* in the *thread* of *whalebone*, as well as in other *threads* of the same *class*, we have reason to conclude, that their apparent immobility before they come to that point, while the *slips* continue to move, is also a *stationary* state, during which they continue to receive water, by the increase of *moisture* in the air.

57. The experiments I have now analysed are only one set amongst others which, though made with less accuracy, have given the same general results. These, relating to various  
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kinds

kinds of substances, I intend to repeat, and to have the honour of communicating their results to the Royal Society; and I shall conclude this Paper, with an immediate demonstration, that the *hygroscopic* motions of the *slips* are simple, while those of the *threads* are the combined effects of two opposite causes: which will be a further confirmation of the whole of the above theory.

*On the recoil of hygroscopic threads.*

58. When formerly I concluded from the phænomena of the *water thermometer*, that its *condensations* were the combined effects of two opposite causes, which followed different laws, it was not for having distinguished those two effects; but only because of a small *retrogradation* near the *freezing point*, preceded by a *stationary* state, comparatively with the *march of quicksilver*; but in the case of hygroscopic *threads* and *slips*, in which we have the same phænomenon, the two opposite effects are distinguishable in the *threads*, by one being operated more rapidly than the other. If, for instance, I transport from a drier to a damper place (or inversely) the two kinds of *quill* hygrosopes, the *slip* proceeds in an *even* course to a certain point, where it remains *fixed*; but the *thread* moves in an *interrupted* manner also to a certain point, whence it *recoils*. If that experiment is made within the limits of the *stationary* state of the *thread*, it may *recoil* as much as it has gone the other way, and be fixed at the same point in both places. The case of the *slip* of *quill* is common to every *slip*, and that of its *thread* to all others which have a quick motion. Here then we have *separately* the *two* effects of *moisture* on the *threads*; that on the *fibres* themselves is the *soonest* produced, and at first predominates: the *slowest*,  
by

by which afterwards the first produced is more or less *compensated*, is that operated on the width of the *meshes*; and it is because the last of those effects is the only one that can affect the *length* of the *slips*, that, in every change of *moisture*, they move *evenly*, without any *recoil*.

59. To that demonstration of the existence of *two* opposite effects of *moisture* in the *threads*, I shall now only add an example of a similar phænomenon, in which the *causes* also are visible. The *compound frames*, mentioned in § 24. are formed of two *glass* rods 4 feet long, fixed together at the bottom and the top. A thin slip of *brass* of determined length, fixed to one of the *glass* rods towards the top, comes down from thence, passes over a pulley at the bottom, and turns up for half an inch. To this end of the *brass* slip is fixed the lower end of the hygroscopic substance, the upper end of which is connected with the *index* of the instrument. It is by that means that, whatever be the changes of *heat*, provided they are *slow*, the lower end of the hygroscopic substance remains sensibly at the same distance from the axis. But if I take that instrument out of water at a low *temperature*, and plunge it immediately into *warmer* water, the *index* instantly moves as if the hygroscopic substance had lengthened; which is the effect of the *brass* slip dilating sooner than the *glass* rods; then the *index recoils*, and this is the effect of a *slower* dilatation of the *glass*.

### *Conclusion.*

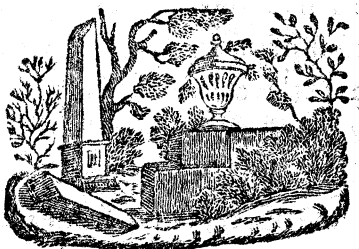
60. I have concentrated in these pages an account of twenty years assiduous labour in *hygrometry*, mostly occasioned by the anomalies of the *hygroscopic threads*; and the principal results have been, some determinations of the four principles that  
directed



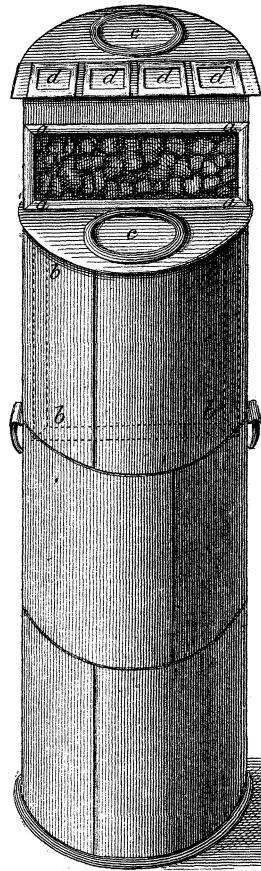
directed me from the beginning, which now are as follows. 1st, *Fire*, as cause of *heat*, is a sure, and the only sure, means of obtaining *extreme dryness*: this is produced by *white heat* in every *hygroscopic* substance that can bear it; and it may be thus transmitted to the *hygrometer*. 2d, *Water*, in its liquid state, is a sure, and the only sure, means of determining the point of *extreme moisture* on that instrument. 3d, It is not to be expected, *a priori*, of any *hygroscopic* substance, that its *changes* be proportional to those of *moisture*; but it may be affirmed, that no *fibrous* or *vascular* substance, taken *lengthwise*, is proper for the *hygrometer*. 4th, A means of throwing light on the *march* of a chosen *hygrometer*, may be, to compare it with the correspondent changes in *weight* of many *hygroscopic* substances.

61. From those determinations in *hygrometry* some great points are already attained in *hygrology*, *meteorology*, and *chemistry*, of which I shall only indicate the most important. 1st, In the phenomenon of *dew*, the *grass* often begins to be *wet* when the *air*, a little above it, is still in a middle state of *moisture*; and *extreme moisture* is only certain in that *air*, when every solid exposed to it is *wet* (§ 28.). 2d, The *maximum* of *evaporation* in a close space, is far from identical with the *maximum* of *moisture*; this depending considerably, though with the constant existence of the other, on the *temperature* common to the *space* and to the *water* that evaporates (§ 30.). 3d, The case of *extreme moisture* existing in the open transparent air, in the day, even in time of *rain*, is extremely rare: I have observed it only once, the *temperature* being 39°. 4th, The *air* is *drier* and *drier* as we ascend in the atmosphere; so that in the upper attainable regions, it is constantly very *dry*, except in the *clouds*. This is a fact certified by M. DE SAUSSURE's observations and mine. 5th, If the whole atmosphere passed

from *extreme dryness* to *extreme moisture*, the quantity of *water* thus *evaporated* would not raise the *barometer* as much as half an inch. 6th, Lastly, in chemical operations on *airs*, the greatest quantity of *evaporated water* that may be supposed in them at the common *temperature* of the atmosphere, even if they were at *extreme moisture*, is not so much as  $\frac{1}{1000}$  part of their mass. These two last very important propositions, have been demonstrated by M. DE SAUSSURE.



*Fig. 1.*



*Fig. 2.*

